EDITORIAL COMMENT

This department of California and Western Medicine presents editorial comment by contributing members on items of medical progress, science and practice, and on topics from recent medical books or journals. An invitation is extended to every member of the California and Nevada Medical Associations to submit brief editorial discussions suitable for publication in this department. No presentation should be over five hundred words in length.

Convalescent Serum Therapy.—The currently reported and apparently reluctant conclusion of Doctors Aycock, Park, and others [1] that convalescent human serum was without verifiable therapeutic value during the recent eastern epidemic of poliomyelitis, is without element of surprise to professional bacteriologists in touch with the rapidly growing complexities of their subject. No progressive bacteriologist today would dare predict whether poliomyelitis convalescent serum should logically succeed or equally logically fail. Predicted success would be but affirmation of personal endorsement of the historic "static" theory of microbic specificity. Predicted failure would be but rhetorical overemphasis of the newly suggested "dynamic" microbiology.

Fifty years ago pathologists were introduced to a new biologic world and intuitively read into the newly discovered disease germs concepts and generalizations drawn from racial experience with higher animals and higher plants. Centuries of experience had taught modern man that, from generation to generation, higher plant and animal species are very nearly static in anatomical structure and physiologic peculiarities. Since the Middle Ages rats had never been known to transmute themselves into frogs, nor to disintegrate into a swarm of locusts. Without realizing the necessity of experimental proof, an equal racial stability was assumed for pathogenic microörganisms, which assumption became the major premise for subsequent epidemiologic and therapeutic deductions. Specific vaccines and specific antiserums had predictable therapeutic value because the corresponding viruses were assumed to remain qualitatively static in antigenic individuality under any and all test-tube conditions and conditions of natural or artificial infection.

Much as conservative theorists [2] may find debatable details in the newly suggested "dynamic" microbiology, test-tube "dissociation" of pure cultures of pathogenic microörganisms into two or more morphologic, tinctorial or biochemic variants is no longer in dispute.[3,4,5,6] That the same infectious agent injected into two different animal species may acquire qualitatively different vaccination potentials is now well confirmed, with similar "biomutation" even alleged in different organs of the same animal. That the spirochetes of relapsing fever are of different specificities in successive waves of the same infection is now generally recognized, with equally marked differences in antigenic specificity alleged between primary and tertiary *T. pallidum.* No up-to-date bacteriologists today would dare predict that second-wave convalescent relapsing fever serum would necessarily have a verifiable specific therapeutic effect against an initial injection with *S. recurrentis,* nor that tertiary convalescent syphilitic serum could be assumed without convincing clinical tests to be a logically effective therapy against primary or even secondary syphilis.

Nothing, of course, is known as to the degree of "biochemic plasticity" or "chemomutation" of the poliomyelitis virus during the course of a natural human infection. Any specific therapy which asumes 100 per cent antigenic stability for this virus, however, is little more than a justifiable clinical gamble.

Due to the numerous debatable questions between the radical and conservative schools of bacteriology, clinicians must receive and try specific vaccines and specific antiserums with an open mind, and judge their predictable efficacy solely from adequately controlled clinical data.

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1 Science Service Report, Science (Supplement), 74:10 (Oct.), 1931.
2 Winslow, C. E. A.: Science, 75:121 (Jan. 29), 1932.
3 Hadley, P., Delves, E., and Klimik, J.: J. Infect. Dis., 40:1, 1927; 48:1, 1931.
4 Miller, F. R.: Science, 74:343 (Oct. 2), 1931.
5 Mellon, R. R.: Proc. Soc. Exper. Biol. and Med., 29:206 (Nov.), 1931.
6 Sherman, J. M., and Safford, C. E.: Science, 74:602 (Dec. 11). 1931.

The Pharmacologic Action of Chiniofon.—Commercial drug manufacturers generally fail to offer the medical profession full and satisfactory pharmacologic data on new drugs exploited for some specific medical purpose. Adequate toxicity data are usually lacking. This important information frequently is supplied by outside research laboratories only after some trouble or other has arisen in connection with the use of the drug. For example, the toxic action of cinchophen (New and Nonofficial Remedies) ("Atophan") on the liver is only now being properly studied following the many cases of liver damage and even death that have apparently resulted from the use of the drug, although when it was originally introduced the absence of any significant information on toxicity as supplied by the manufacturers would naturally lead the medical profession to presume that the drug was relatively nontoxic. It is my opinion that drug manufacturers should be held responsible for the untoward effects of drugs introduced by them for which important data, especially on toxicity, is not afforded.

The recent report by Schwartze and Billinghurst [1] on the pharmacologic action of chiniofon is another illustration of this proposition. Chinio-

1 Schwartze, E. W., and Billinghurst, G. A.: Studies on the Pharmacologic Action of Chiniofon Other Than Its Amebacidal Properties, J. Pharmacol. and Exper. Therap., 45:273 (July), 1932.